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THE MIND OF ORGANISMS: SOME ISSUES ABOUT ANIMAL COGNITION

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Sense sure you have,
Else could you not have motion.
Hamlet, III, 4

INTRODUCTION

The study of animal behavior and intelligence has a fairly long tradition, starting with Romanes naive mentalism. With a few noble exceptions, like Tolman and Köhler, psychological research on animals has been dominated by the behaviorist paradigm, and only in the last fifteen years has there been a substantial growth of interest in the analysis of cognitive processes in animals. This renewed impetus towards a cognitive approach, as opposed to a strict behaviorist perspective, resulted from both internal problems and external influences: on the one hand, there were difficulties in explaining all instances of behavior within the traditional S-R approach; on the other, mental concepts were gaining a new scientific respectability, thanks to the development of human cognitive psychology and artificial intelligence.

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In the late sixties, the powerful influence of behaviorism on animal psychology began to decrease, as a consequence of a variety of empirical data, which proved difficult to explain, or even contradicted the fundamental assumptions of S-R theories. Phenomena such as autoshaping, selective attention, conditioned learning of taste aversions, and preferential learning of some responses showed that the traditional laws of learning were inadequate to explain every conceivable case of learned behavior, in humans as well as in other animal species.

While learning theory continued to evolve in response to empirical challenges, trying to accommodate all the new findings within the classical conception through *ad hoc* adjustments of the accepted laws, a growing number of comparative psychologists felt that the basic assumptions of behaviorism needed to be re-examined.

Along this line, a number of studies questioned the universality of the S-R laws of behavior at both the intra- and the interspecific level, focusing on the relevance of biological factors in controlling behavior. This area of study stimulated debate on biological constraints and adaptive specializations in learning (Bolles, 1970; Hinde & Stevenson-Hinde, 1973; Rozin & Kalat, 1971; Seligman, 1970; Shettleworth, 1972), promoting concern for functional approaches to the study of learning (Hollis, 1984; Staddon, 1983).

A different line of research has attempted to apply the tools of human cognitive psychology to the study of animal behavior. In recent years, a number of systematic attempts have been made to explore this possibility in a comparative frame of reference. This is the case for comparative analyses of short and long term memory (Van der Wall, 1982; Grant, Brewster, & Stierhoff, 1983; Vaughan & Green, 1984; Roberts & Van Veldhuizen, 1985), studies of cognitive maps (O'Keefe & Nadel, 1978; Gaffan & Gowling, 1984; Gould, 1984, 1986), works on categorization and concept formation (Herrnstein, 1984, 1990; Lea, 1984), studies on linguistic abilities of different species (Ristau & Robbins, 1982; Herman, 1986; Schusterman & Gisiner, 1988; Pepperberg, 1991), and research on natural communication systems in animals (Snowdon, 1987). Although not yet conclusive, the results of these studies are beginning to take a coherent shape, providing important information for answering questions about the evolution of cognition, and suggesting new and stimulating directions for future research. It is with this approach that we are concerned here.

This paper is neither a review of all relevant work in animal cognition, nor a complete, detailed survey of the theoretical stands taken by researchers in the field: even though the discipline is still young, a similar endeavor would require at least a book size work. Our aim is rather to present, analyze and discuss the basic assumptions of animal cognition, focusing on those aspects that appear to be central today, and will presumably continue to be so in the near future. The questions are: What

do those who study animal cognition intend to achieve? And why? And how?

In Section 1, we state the main goals of those who study animal cognition, and argue that this discipline has an intrinsically comparative nature. In Section 2, we delineate some classical objections to cognitivism, show that they have been overcome by present day methodology, and introduce the notion of representation as the basic element of cognition. In Section 3, we introduce the view of representations as mental states, i.e., states endowed with content; an alternative perspective, based on the notions of form and formal manipulation, is presented in Section 4. Finally, in Section 5 we draw some conclusions.

1. THE WHY AND THE WHAT OF ANIMAL COGNITION

> *What a piece of work is a man!*
> *how noble in reason! how infinite*
> *in faculty* [. . .]*! The beauty of the*
> *world, the paragon of animals!*
> Hamlet, II, 2

While the study of human cognition arose as a clear-cut break with the behaviorist paradigm, animal cognition, partly due to the nature of the available data, necessarily maintains a certain degree of continuity with the traditional methods. The cognitive approach brings to the comparative psychologist a further set of tools for the formulation of theoretical models of animal intelligence. In the words of Roitblat, Bever, and Terrace (1984),

> Animal cognition is concerned with explaining animal behavior on the basis of cognitive states and processes, as well as on the basis of observable variables such as stimuli and responses. (p. 1)

Whatever position one may adopt towards cognitive states and processes, it is clear that the main reason for attributing cognition to animals is that we, as humans, do experience a mental life. While such an attribution is in agreement with a unitary and evolutionary view of organisms, it introduces an element of anthropomorphism, which has often motivated suspicion or rejection by scientists. However, animal cognition does not imply a straightforward transfer to animals of models of human thought, which would indeed be unjustified; rather, it is to be taken as a source of possible explanatory hypotheses about the unobservable determinants of animal behavior, which are then to be tested through a strict empirical methodology. It is expected that by careful experimental control the anthropomorphic component of cognitive models can be made harmless—as harmless as the anthropomorphic component of concepts like force and energy in classical physics.

Given that the prototype of cognition is, by definition, human thought, animal cognition appears to be an intrinsically comparative study of intelligence: the direction of comparison goes from humans to animals, then back again to humans. In fact, even though the fundamental concepts of the cognitive approach originate in human psychology, we expect that they will be substantially enriched and refined through the attempt to apply them to other species. Although the physical continuity between humans and the other species was accepted more than one century ago, the problem of the continuity of mental capabilities has not yet been satisfactorily solved.

But what are the fundamental concepts of a comparative study of cognition? As Roitblat says (1987),

> Comparative cognition is the study of the mind of organisms and the ways in which those minds produce adaptive behaviors. It is an approach to understanding behavior that emphasizes what animals know and how they use that information in guiding their behavior. Comparative cognition seeks to understand how animals acquire, process, store, and use knowledge about their world. (p. xii)

As already remarked, cognition is concerned with explaining behavior not only through observable variables like stimuli and responses, but also on the basis of cognitive states and processes, which are not directly observable. Apparently, the goal has not changed since the time of Romanes. But what sounds similar need not be the same. There is no room in contemporary "cognitivism" for naive anthropomorphism; as we shall argue in the next section, the criticisms made to Romanes' easygoing approach are not pertinent any more.

There are basically two orders of considerations that motivate a cognitive approach to the study of animal behavior. The first one, as we have already suggested, arises from the limitations of behaviorism, and views cognitive concepts as hypothetical constructs that might provide better explanations of empirical data. From this standpoint, cognitive science does not differ from any other natural science, in that it postulates unobservable entities to explain the regularities of observable phenomena. Such entities are justified when they provide economical and general interpretations of complex findings, and produce predictions that are experimentally testable.

But comparative cognition has also a completely independent motivation, which is often overshadowed by the previous one. As regards the human species, cognition is not so much an explanatory construct as a plain matter of fact: mental states are part of subjective reality before entering the theoretician's tool kit. But the mind is a very complex biological entity, and Darwin teaches us that any such thing stands in need of an evolutionary explanation: Where does cognition come from?

How did it evolve? Is *Homo sapiens* the only cognitive organism on the earth?

So, there are two sides to cognition: it is a tool for understanding behavior, but also a phenomenon to be understood in its own right; and we believe that a comparative approach should be concerned with both issues.

At this point, a number of questions arise. Are there real methodological problems with the use of mental notions in natural science? If not, what makes the mental different from the nonmental? How can mental processes be described? And then: What is the adaptive value of cognition? Are there species-specific differences in mental processes?

In the following sections, we shall consider possible answers to some of these questions.

2. NATURAL SCIENCE AND THE CONCEPT OF MIND

Behaviorism emerged as a reaction to the fuzzy, prescientific use of mental terminology in "internal eye" psychology. Mental concepts were regarded to be incompatible with the materialistic stand required by a mature scientific discipline, and were viewed as uneconomical and superfluous in a science of behavior. Moreover, mental explanations were considered to be unfalsifiable, in that it was always possible to find one that fitted any experimental data.

As documented in the scientific literature (Sober, 1983), the revival of a science of the mind was made possible by the overcoming of these objections. Here we shall run quickly through this matter, focusing on a few points which are particularly relevant for our goals.

A first objection to the use of mental concepts in science was that mental processes are not physical. A similar assumption is certainly part and parcel of the Cartesian doctrine, but it is by no means a necessary corollary of the concept of mind. As remarked by Place back in 1956, it is perfectly sound to assume that typical mental features, like consciousness, are features of neurophysiological processes: the mind need not be less physical than any other process studied in natural science. In talking about the mind we must be very careful, because in ordinary language the terms "physical" and "mental" are opposite; it is therefore up to natural science to construe the notion of mental process so that it is a special kind of physical process.

When we accept this assumption, we might be tempted to get completely rid of any notion of mind and to consider only neurophysiological phenomena. In fact, this position is advocated by the so-called "eliminativists," like Churchland (1981). The main problem with this approach is that it fails to identify the characteristic properties of the mental. Given that mental processes are neurophysiological, not all neurophys-

iological processes need to be mental; but how can we find out which ones are, if we do not have an independent theory of the mind? As remarked by Sober (1983), there is a big difference between explaining the mental, and explaining it away.

It is important to note that leaving the neural level to deal with mental states does not force us to analyze the subjective quality of conscious experience. Phenomenological issues, put forward by Griffin as the core of cognitive enquiry (1978, 1981, 1984), pose problems far beyond the present possibilities of experimental research. But, as we shall see in the following sections, cognitive science has developed concepts and methods to deal with the mind from an objective, rather than subjective, standpoint.

The goal is therefore to build an independent theory of mental processes, by putting forward a number of hypothetical constructs for the explanation of behavior from an objective standpoint. As stressed by Chomsky (1959), there is no special problem in postulating unobservable entities in scientific theories; almost any science deals with hypothetical entities that can only be inferred from observable events.

A frequent objection to the use of mental explanations is based on the well-known Morgan's canon (1894), stating that:

> . . . in no case may we interpret an action as the outcome of the exercise of a higher psychical activity, if it can be interpreted as the outcome of one which stands lower in the psychological scale. (p. 53)

However, in 1903 Morgan himself added that:

> . . . the canon by no means excludes the interpretation of a particular activity in terms of the higher processes, if we already have independent evidence of the occurrence of these higher processes in the animal under observation. (p. 59)

Again we have a situation common to many sciences. A general theory, accounting for a whole set of phenomena through higher level concepts, is preferable to a theory that explains the same phenomena by lower level processes, but requires several *ad hoc* adjustments to encompass all of them. In fact, one of the goals of the study of comparative cognition is to provide general explanations of a wide range of observable behaviors.

Perhaps a more severe objection, put forward by Skinner (1964), is that mental explanations can always be made to fit any experimental finding, thus dooming mental theories to be unfalsifiable. In fact, this appears to be an actual risk for cognitive theories, that have a very complex equipment of unobservable entities. Therefore, comparative cognition must take great care to avoid falling into this trap. This point will be considered in the following sections.

To summarize, the cognitive approach is based on two fundamental assumptions. The first assumption is that cognitive processes are physical

and biological, in that they are fully realized in the nervous system of the organism. The second one is that cognitive processes can be described at an abstract level, making no reference either to the specific quality of the subjective experience of the organism, or to the processes taking place at the neural level.

It has been argued that to keep the concept of mind in a scientific context it is necessary: (i) to identify and define instances of mind, and to establish a set of procedures and empirical markers with some degree of consistency; (ii) to show that the concept of mind will serve to more efficiently integrate and organize existing information; (iii) to demonstrate that the formulation permits the derivation of specific, testable predictions about the presence or absence of mind and its influences on behavior (Gallup, 1982). These recommendations are, however, very general and contain neither reliable nor simple formulas for deciding if and when we should use cognitive terms when dealing with animals.

In fact, in order to explain behavior, many contemporary comparative psychologists use a mass of technical terms that have an intrinsic cognitive connotation, even if they are not always defined in a precise way. A list of such terms includes cognitive map, perception, memory, concept, representation, expectation, rule, goal, behavior plan, linguistic ability, and intelligence.

Although these terms cover a wide range of different ideas, they share the common underlying notion of representation, which, therefore, qualifies as the central concept of cognitive theories. In fact, two different views of representations have been adopted in animal cognition. The first approach, presented in the next section, regards representations as mental states, defined by a mode and a content, both involved in causing behavior. Typical mental states are beliefs and desires about objects, facts and events in the environment. According to the second perspective, known as information processing psychology (Section 4), representations code information about the environment, and their ability to mediate between stimuli and responses relies upon transformations performed by computational processes, which are sensitive to their formal structure.

3. THE SEMANTIC MIND

A possible approach, which is gaining favor especially within cognitive ethology, is to regard representations as particular types of internal states, such as beliefs and desires, that can be held by organisms. In analytic philosophy, such states are called mental or intentional, and their characteristic property is that they are about objects and states of affairs in the outside world: for example, a belief is always the belief that something is the case, and a desire is the desire that something be the case. It is important to note that the term "intentional," here, does not mean voluntary or purposive as in everyday English; following a tradition started

by Brentano and Husserl, and continued by a number of contemporary philosophers of mind, it just means about something, and, therefore, has a broader sense. What we call "intentions" in everyday language is just one possible form of intentionality.

When representations are regarded as mental states, their essential feature is content. Representations have a content, in that they represent something: objects of the external world, relationships among objects, facts, events, etc. In other words, representations hold a semantic relationship with the environment.

Mental states are made up not only by a content, but also by a mode (Searle, 1983). Examples of modes are: to believe that, to desire that, to see that, to intend to, to fear that, etc. Note that, in terms that should be more familiar to comparative psychologists, holding a belief is nothing more than possessing certain information about the environment, while a desire is just a goal or a purpose. Two different mental states may have distinct modes, while sharing the same content. For example, the belief that one's offspring is safe and the desire that one's offspring be safe are two distinct mental states, with equal content and different modes.

The idea of a semantic relationship between representations and reality originates in human conscious experience: for example, the experience we have when we see something is that there are real objects out there, showing certain properties and relationships. In fact, consciousness is taken as the central issue in the study of cognition by Griffin (1978, 1981, 1984), who defines cognitive ethology as the study of the mental experiences of animals.

Even if one accepts that representations presuppose conscious experience, it is not the subjective quality of the experience itself that is under investigation. In fact, such a subjective quality is impossible to assess: how could we possibly know what it is like to be a bat? (Nagel, 1974) Fortunately, the aim of a scientific study of the minds of other animals is not to find out what it is like to be a certain type of animal, but rather to clarify how mental states cause observable behavior. In order for mental states to have an explanatory role, their power to produce behavior has to be a function of their constitutive features, i.e., their content and mode. But content and mode can be defined without trying to make the actual quality of experience explicit. Consider for example the perception of colors. The ability of an animal to discriminate objects of different colors, plus the presence of cones in the retina, would be considered as sufficient evidence that the animal has color vision. Even if we have no idea of the exact nature of the experience of the animal when it is looking at a red triangle, we can take colors into account when describing the content of the animal's visual perceptions.

A characteristic property of mental states, like beliefs and desires, is that they exhibit a logic. For example, keeping in mind the definitions of belief and desire given above, from the belief that there is an intruder near the nest, and the belief that intruders are dangerous for the offspring,

follows the belief that the offspring is in danger. The attribution of logical capacities to animals may appear as a piece of unjustified anthropomorphism. But this is not necessarily the case, as simple logic does not require high level abilities, like that of reflecting upon one's own beliefs and concepts, which might well be specific to the human species. As Griffin (1991) reminds us, complex phenomena like self-awareness and thinking about the process of thinking itself are by no means necessary components of cognition: in fact, to think that they are so would be the real anthropocentric mistake.

The view of representations as mental states, which is traditional in analytic philosophy, is far less accepted in cognitive science. One common criticism is that notions like belief and desire are metaphoric and, while used in everyday "folk psychology," have little to share with real science (Stich, 1983). However, the work of philosophers like Dennett (1987) and Searle (1983) and pioneering research in animal cognition show that mental states, and in particular beliefs and desires, can be employed as useful explanatory tools and undergo rigorous scientific investigation.

The fact that "belief" and "desire" are part of the folk vocabulary used to describe everyday behavior does not mean that the same terms cannot be used technically. It is inevitable for a science of the mind to have some overlap with everyday language. Similarly, linguists use terms like "sentence" and "name" in a strictly technical way, and nobody thinks that they are producing "folk linguistics"; the same is true for such terms of physics like "force" and "energy." Furthermore, terms like "belief" and "desire" are by no means metaphors. The ascription of mental states to an organism, in order to explain its behavior, is meant to be literal, not metaphoric, in that it is assumed to describe—at a high level of abstraction—a real physical state of the organism. Once more, there is no difference with respect to other sciences: to say that a body moves under the action of gravitational force is a literal statement, not a metaphoric one, even if the notion of force is a theoretical construct.

Clearly, before beliefs and desires can be used to explain behavior, we need a general theory of mental states. Here we shall consider two different approaches: Dennett's intentional stance and Searle's biological naturalism.

In the field of animal cognition, the best known approach to intentional explanation of behavior is that proposed by Dennett (1987), under the name "intentional stance." Essentially, the intentional stance is the standpoint of the scientist who seeks to explain behavior as a rational consequence of beliefs and desires ascribed to the organism.

The role of rationality is to dictate how beliefs and desires interact in determining behavior: it is assumed that an organism acts in order to fulfill its desires on the basis of its beliefs. As Dickinson says (1988),

> In general, I assume that an intentional account of behavior is justified if that behavior can be shown to be dependent on, in the sense of being

> a rational consequence of, a set of beliefs and desires about the world. (p. 307)

It is essential that the explanations in terms of mental states are not simply *post hoc* reconstructions. As remarked by Bennett (1991), the belief-desire-behavior triangle is, so to speak, an equation with two unknowns: one can always find many different belief-desire pairs that explain any given behavior. Therefore, we need some criterion to attribute beliefs and desires in advance, in order to predict a forthcoming response; the validity of the attribution will then be tested by observing the behavior actually occurring.

Of course, it is not possible to give a list of observable features that are necessary and sufficient for an organism to entertain a specific belief. However, as holding a belief means to possess certain information about the environment, we can try to attribute certain beliefs to an animal when they can be the result of its learning history and of its present situation, given the characteristics of its sensory apparatus.

With desires we face a similar problem. From a functional standpoint, desires act like motivational states in producing behavior. They differ from simple motivations in that, having a content, they can combine with beliefs, thus determining in a flexible way a response that fits the situation as represented by the organism. Therefore, when we attribute a desire we must take into account both the basic motivational states that the animal is assumed to have, and the possibility that it combines with the animal's beliefs.

An example of this methodology can be found in Dickinson's experiments on intentional behavior in rats (1988). In one of these studies, hungry rats were trained to pull a chain in order to obtain sucrose solution, and to press a lever to obtain food pellets. By changing the motivational state from hunger to thirst, it was found that the rats preferred pulling the chain to obtain sucrose solution, provided that they had previous, independent experience of the different effects of sucrose solution and food pellets on the state of thirst. These results can be accounted for in terms of rats holding beliefs and desires (Figure 1), the content of which is directly determined by the experimental conditions in the following way:

> As regards desires, the motivational states of hunger and thirst were produced experimentally, via food and water deprivation. Furthermore, the experimental procedure allowed the rats to learn the value of both food pellets and sucrose solution in relieving hunger, and of sucrose solution in relieving thirst. Therefore, we are justified in attributing to hungry rats the desire for either food pellets or sucrose solution, and to thirsty rats the desire for sucrose solution only.
>
> As regards beliefs, the experimental procedure was designed to let the rats acquire the information that pressing the level caused the delivery

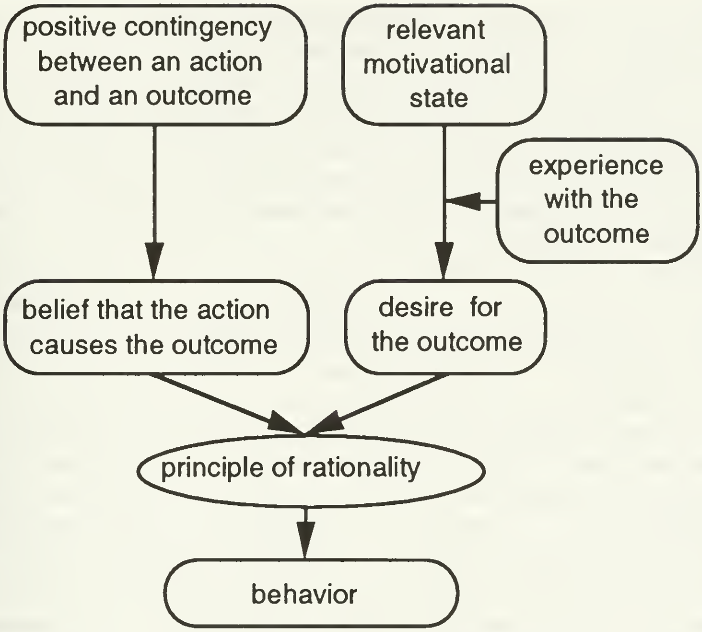

FIGURE 1. An experimental application of the intentional stance.

of food pellets, and pulling the chain caused the delivery of sucrose solution.

Having thus attributed beliefs and desires to the animals, the principle of rationality leads us to predict that thirsty rats will try to fulfill their desire to get sucrose solution by pulling the chain. This prediction was confirmed by the observed behavior.

This experiment deserves a few words of comment. First, it is remarkable that even simple instrumental behavior supports an intentional account; however, as stressed by Dickinson himself, particular care is required in designing experiments in order to evaluate competing mechanistic and intentional explanations. Second, one should not expect that representations spring up by themselves in the animal's mind; sufficient experience with the relevant aspects of the world is crucial to support the content of both beliefs and desires. For example, in the reported study previous experience with the effects of the reinforcers was essential to turn the pure motivational states into actual desires.

The intentional stance is by no means confined to laboratory experiments; in fact, it has more often been adopted in cognitively oriented field research (Ristau, 1991). Indeed, we think that a number of results reported in the literature are suitable for an intentional interpretation; this seems to the case also for simple organisms, like honeybees.

In a series of extremely intriguing experiments on honeybee cognition,

Gould and Gould (1988) showed that the bees' ability to use their maps of the territory apparently goes beyond simple navigation. It was observed that dance attenders were not recruited by dances indicating that flowers were located in an adjacent lake, whereas they were recruited normally by dances indicating an equally distant location along the shoreline. A possible interpretation proposed by Gould and Gould was that the location in the middle of the lake must, in a sense, have "sounded unplausible" to the bees.

This interpretation could be easily cast into intentional terms. Given the nature of the motivational state of bees, we can assume that all bees ready to leave the hive hold a comparable desire to reach the flowers. What inhibits the recruitment appears to be the belief that no flowers are to be found in the middle of lakes.

As observed by Gould and Gould, it is not easy to imagine what kind of selective pressure might have promoted the ability to discard, on the basis of an individually constructed map, the information obtained from the dancers. In fact, there is no experimental evidence, and no theoretical reason as well, supporting lying and deceit in honeybees. This problem is related to the more general question of what might be the adaptive value of cognitive processes. At the present stage, it is only possible to attempt a few speculations. On the one hand, the ability to disbelieve a message when it clashes with previously acquired information has an adaptive value not only in case of deception, but also if messages are prone to errors. On the other hand, it is possible that such an ability has no value of its own, but is a consequence of selective pressure toward the more general capacity to hold beliefs about the environment.

From a methodological point of view, Dennett's intentional stance is an instrumentalist position, in that it is neutral with respect to such issues as the real nature of mental states, their experiential correlates, and their relationship with the actual causes of behavior. The instrumental nature of the intentional stance becomes especially clear if one considers the role of the principle of rationality. Principles of this kind are common in science. For example, predictions of the fate of physical systems can be based on the principle of minimum energy: if a spherical body is allowed to move freely in a concave container, sooner or later it will stand still at the bottom of the container, having reached a state of minimum energy in the gravitational field. The minimum energy principle thus allows one to predict the final equilibrium state in a synthetic way, without bothering about how the state is reached. This kind of physical explanation is clearly not causal, because there is no assumption that a "tendency to minimum energy" is acting on the body. However, this does not rule out the possibility of explaining the same phenomenon causally, which can be done by taking gravitational force and friction explicitly into account. In fact, the minimum energy principle can be derived from the basic laws of physics: its use does not imply that one

gives up the assumption that all physical phenomena have a causal explanation.

On the basis of these considerations, it is natural to wonder whether we can replace the principle of rationality with a causal account. According to Bennett (1991), intentional explanations are noncausal: they should be regarded as simple, synthetic tools for making predictions about behavior; causality only makes sense at the neural level. A substantially different standpoint is taken by Searle (1983), who argues that mental states are not only explicative tools, but rather real states endowed with causal power.

According to Searle, intentional states are a particular kind of physical state of the nervous systems, and as such can cause other intentional states and, eventually, behavioral responses. What characterizes intentional causation with respect to classical physical causation is that there must be a certain kind of relationship between mode and content of the causally related intentional states. For example, thirst may cause an intentional act of drinking because thirst involves a desire to drink, which is satisfied by the act of drinking itself. This kind of explanation is coherent with the traditional requirements of natural science. Rationality appears to be an emergent property of intentional causation, and the principle of rationality is therefore a derived law, like that of minimum energy.

Between Dennett's instrumentalism and Searle's realism there is indeed a profound philosophical difference. But this does not necessarily imply a comparable difference in the explanation of behavioral data. Beliefs and desires, whether considered as instrumental attributions or as descriptions of real physical states of an organism, lead to the same predictions about the organism's behavior. At the present state of cognitive science, the question of which of the two approaches should be adopted is a matter of personal philosophical position, and cannot be settled on the basis of observable data.

However, beyond strictly philosophical matters, Searle's work on intentionally presents many ideas that might prove important for developing a general theory of cognition. In particular, two points are worth discussing here.

The first is that although scientists can only describe the content of mental states through language, such contents need not be realized in linguistic form in the mind. Language is necessary for us to describe the representations held by other organisms, but it is not necessary in order for representations to be realized in the brain. When we say that an animal perceives an intruder, we do not mean that the animal entertains a mental sentence like "There is an intruder in front of me"; rather we mean that the animal is in a neural state related to the world in a way that an external observer can describe by the reported sentence.

The second important point is that allowing for representations in the

mind does not mean that every process going on in the brain is representational. Rather, representations presuppose a rich repertoire of nonrepresentational capacities as a necessary background. Let us consider Sober's example (1983) of a dog, Fido, recovering a bone previously hidden under a tree. Fido's behavior can be accounted for in terms of the belief that there is a bone under the tree and the desire to get the bone. Therefore, we assume that its mental states contain representations of a bone and a tree. Such representations are possible because Fido is able to discriminate bones and trees from other types of objects. However, its ability to discriminate bones and trees, which is a necessary precondition for holding representations about bones and trees, is not itself based on representations.

Searle's idea is that without a rich repertoire of such nonrepresentative capacities, that he calls the Background, we cannot even start to form representations about the world. After being able to recognize stones, tables and the "on" relationship between two objects, we can entertain the thought that a particular stone is on a given table. But the ability to recognize a stone is not itself based on beliefs about stones.

When we attribute to Fido the belief that a bone is buried under the tree, we give for granted that it is able to recognize a bone. As Sober remarks, the use of the term "bone" in describing Fido's belief does not imply that we attribute to it our knowledge of bones, e.g., that bones are part of an animal's skeleton, that they can be used to make a tasty broth, etc. Fido's representations must be considered to be relative to its Background, not to our Background and general knowledge.

This kind of species relativism is extended by Millikan (1986) to the very notions of belief and desire. In commenting on Gould and Gould's researches on honeybee cognition, she says that:

> . . . it is unlikely that there is any distinction *within* the performing bee to correspond to the distinction between belief and desire—unlikely that the bee either believes or desires anything in the human way. (p. 72)

Perhaps it is not necessary to go this far. As we have already said, it is not the experiential quality of beliefs and desires that matters, but rather their role in causing behavior. Fido does not possess the same information we have about the world, but certainly it has some information; the content of its beliefs will be "doggish," but they are beliefs after all.

The standpoint just outlined suggests that representations are the tip of a nonrepresentation iceberg. It follows that cognitive science has two concerns: first, the role of representations in producing behavior, which we have discussed in the present section; second, the nonrepresentative process that generate representations. We shall come back to this point in the next section, devoted to the paradigm of information processing psychology.

4. THE COMPUTING MIND

When dealing with representations, it is traditional to distinguish between content and form. While issues about content have been extensively investigated in philosophy, cognitive psychologists have devoted their attention mainly to form.

The role of form is a central concern of information processing psychology (IPP), which regards mental processes as a flow of information through a number of cognitive subsystems. A pioneering effort in this direction is Broadbent's model of memory (1958). It is assumed that in any subsystem information is coded in a suitable way, and that cognitive processes can be regarded as transformations acting on coded information. As remarked by Yoerg and Kamil (1991),

> The task of the cognitive psychologist from an information processing perspective is to determine the nature and organization of the processes which transform, encode, represent, and use information from the external (or internal) world to produce behavior. (p. 279)

Possibly the main reason for the success of IPP has been the availability of rigorous mathematical tools derived from information theory (Shannon, 1948). A further impetus came from computer science, and in particular from artificial intelligence. According to Newell and Simon (1978), any intelligent system, either natural or artificial, is a physical symbol system, i.e., a physical system whose states are symbolic structures, and whose processes are computations performed on such structures. In a physical symbol system, symbolic structures play the role of representations; however, computations are sensitive only to the form of representations, not to their content.

This version of IPP is substantially equivalent to the philosophical position originally put forward by Putnam in 1962 under the name of functionalism, and developed in a series of papers reprinted in Mind, language and reality (1975). According to this view, the brain is to be regarded as a digital computer executing a specific program. The resulting computations transform the stimuli (input) into behavior (output), through a series of intermediate steps. Mental states are simply states occurring in the computations carried out by the brain according to the program. Therefore, for any given organism the goal of psychology is to determine the program executed by its brain. It is important to note, however, that Putnam has not completely changed his philosophical position (1988), reaching the conclusion that functionalism cannot shed any light on the structure and activity of the mind.

Functionalism has become a popular approach to cognition for a number of reasons. As it reduces intelligence to computations carried on by a machine, it is clearly a materialistic approach. The brain is viewed as just one possible kind of machine able to carry on the required computations; functional models are abstract and independent of the neu-

rophysiology of the brain, and, therefore, the present lack of knowledge about brain processes does not bear on cognitive modeling.

From the point of view of animal cognition, the main virtue of functionalism is perhaps that it does not presuppose any kind of subjective experience. Consciousness, if present, is an epiphenomenon, in that it does not contribute in any way to the computational process.

A typical controversy in IPP is whether particular cognitive processes exploit pictorial or symbolic representations, i.e., whether the information is coded as a sort of mental image or rather in a sentence-like form. Questions of this kind may be addressed either at the competence or at the performance level (Airenti & Colombetti, 1991). In the former case, the relevant variable is whether the subject is or is not able to perform a certain task; in the latter, the focus is on variables like the time required to produce a response.

An example of competence oriented research on the nature of representations is provided by the work on category discrimination by pigeons carried on by Pearce (1988). In a number of experiments, Pearce has shown that pigeons learn to discriminate visual stimuli consisting of several bars on the basis of their absolute height, but find it very difficult to discriminate on the basis of the same/different height relationship. Referring to Premack's claim (1983) that the ability to rely on relationships between stimuli is the mark of symbolic representation, Pearce interprets the results of his study as showing that pigeons store visual information in pictorial rather than symbolic form.

Also, performance data have been invoked to support hypotheses about the form of representations. An interesting and well-known kind of experiment studies the ability to recognize different rotations of an image. Shepard and Metzler (1971) showed that the time employed by human subjects to recognize an image as the rotation of another one was proportional to the angle of rotation. This result strongly suggests that such images are represented in pictorial form. A similar set of experiments was carried out by Hollard and Delius (1982) using the same apparatus, task and stimuli on both pigeons and humans. The performance of the two species turned out to be remarkably different. As in the Shepard and Metzler study, the latency of response by humans increased with increasing amount of rotation. On the contrary, the response produced by pigeons did not depend on the rotation angle. The conclusion drawn by the authors was that pigeons and humans use different representational systems.

It is clear from the preceding examples that the aim of IPP is to study how information is encoded by organisms, and to analyze the transformations that operate on such coded representations. However, we think that the very notion of mental transformation is somewhat problematic. Consider for example Gould's researches on the visual perception by honeybees, which are regarded by their author to "shed some light on the nature of the mental transformations honeybees are capable of, though

not as yet on how these transformations are made." (1990, p. 87) In particular, Gould showed that a bee trained to discriminate between two vertically oriented artificial flowers can recognize their right-left mirror image as similar to, even if different from, the original pattern; on the contrary, bees do not exhibit the same ability when confronted with an up-down reversal of the flower.

These results are easy to explain if one assumes that the bee's representation of the flower is pictorial: the representation has to undergo a mental transformation that is analogous to the physical transformation of the stimulus pattern. Gould's findings are then explained by assuming that bees' images can undergo vertical, but not horizontal, mirror transformations. To account for the same results in terms of symbolic representations would be much more difficult, even if not impossible.

However, it is important to stress that within IPP there seems to be an implicit assumption that representations necessarily have either pictorial or symbolic form. But where does this assumption come from? Clearly from the human use of pictures and of language for representing objects or state of affairs. But pictures and words are external carriers of representations, and they do not immediately warrant the assumption that mental representations must be of either kind. Mental representations are not external, and there is no reason to assume that they should mimic some object of our experience.

A similar problem has already arisen in other scientific disciplines. In Newton's times, the assumption that light was made of particles accounted for a number of optical phenomena; however, in the nineteenth century it was discovered that light often behaved as a wave in an elastic medium, in a way that was incompatible with the corpuscular hypothesis. This contradiction remained unsolved until it was accepted that light did not need to have either a corpuscular or an oscillatory nature: it could be something different. The point is that both particles and waves are objects of our everyday experience; but the microscopic structure of light is beyond our direct acquaintance, and so required completely new tools to be described.

Possibly, in cognitive science we are facing a similar situation; maybe mental representations are not like pictures or sentences: they are something else. A concrete example of what they could be like is provided by a recent approach to mental modeling known as neural networks (Rumelhart & McClelland, 1986). Neural networks are mathematical models inspired by the structure of the nervous system: they consist of a large set of units connected by excitatory or inhibitory links of variable strength. Such networks encode information through the strengths associated to the links, and represented as numerical "weights," in a way that is neither pictorial nor symbolic. It is conceivable that a model of this kind may account for Gould's data without resorting to any notion of transformation of representations.

It should be clear by now that contemporary research in animal cog-

nition is following two distinct paths: in general, interpretations in terms of mental states are not integrated with IPP models. This may sound surprising, if one considers that functionalism was first conceived in order to provide a computational basis for concepts like belief and desire (Putnam, 1988, p. 73). But, in fact, the development of IPP has been largely independent of philosophical concerns—and, after all, philosophy has often overlooked the problems of applied research. We think, however, that it is now time to see whether the two approaches can be successfully and usefully merged.

One of the studies considered in the previous section can provide an example of how mental states could be related to information processing. Let us go back to Gould and Gould's finding that honeybees are not recruited by dances indicating that flowers are located in the middle of a lake. We have already suggested an interpretation of this result in terms of beliefs and desires. It is crucial to our interpretation that the bees' representation of the home range can be viewed as a set of beliefs. But where do these beliefs come from?

In general, there is the possibility that a belief is derived from more fundamental ones: recall the example of the belief that the offspring is in danger, which could be derived from the previous beliefs that there is an intruder near the nest, and that intruders are dangerous for the offspring. But then, we are left with the problem of explaining where the previous beliefs come from. It is clear that we cannot assume that all beliefs derive from more basic ones, lest we should face an infinite regression.

When a belief is not derived from more fundamental ones, it has to be the product of some basic Background capacity—to adopt Searle's terminology. Going back to the bees' representation of the home range, it is reasonable to assume that it is the product of a basic ability to represent the spatial structure of the environment. Therefore, while the representation of the home range can be regarded as an intentional state, it has to be the result of a more fundamental, nonrepresentative process.

The problem now is how to explain such a capacity. It seems to us that the real explanation can be given only at the neurophysiological level (Airenti & Colombetti, 1990). But IPP can offer us a possible description of the process that highlights important properties: for example, Gould (1984) presents evidence that the bee's representation of the home range appears to work more like a pictorial map rather than like a series of snapshots of key points along the route.

To conclude, we would like to point out that the IPP approach to the study of animal cognition has at least two main merits. The first is that it allowed scientists to deal with mental features in a very concrete way, helping them to overcome a deeply rooted reluctance. The second, and more important, is that its models are suggestive and have a strong heuristic value: many interesting aspects of animal behavior would not

have been investigated without an information processing frame of reference. However, it should be kept in mind that interpreting mental processes as computations is a metaphor, even if one with a great heuristic power, and not a literal explanation. In fact, there seems to be no reason to assume that the processes going on in the nervous system of organisms are more computational than those occurring, say, in the growth of a plant or in a chemical reaction.

5. SUMMARY

In this paper, a number of issues related to animal cognition have been discussed. In particular, we have argued that:

(1) As it is formulated today, the notion of mind does not commit to the existence of nonphysical entities, and can be investigated within a rigorous scientific framework.
(2) Mental processes can be described at an abstract level, with no appeal either to the quality of subjective experience or to neurophysiological processes.
(3) There are cases in which the behavior of organisms is amenable to an explanation in terms of mental states, i.e., states endowed with content about the external world. However, the existence of mental states presupposes a rich repertoire of nonrepresentative capacities.
(4) Even if for nonhuman animals mental states are hypothetical constructs, in the case of humans their existence is a plain matter of fact, and the challenge for comparative psychology is to establish their evolution.
(5) Explanations of behavior in terms of mental states might well be causal in nature as it is traditional in the natural sciences.
(6) Information processing psychology describes neurophysiological processes in terms of computational analogues. Basic concepts such as mental transformations depend heavily on the tools chosen to describe computations.
(7) It is time to pursue an integration of the approaches based on mental states and on information processing. A promising meeting point could be provided by the nonrepresentative capacities underlying mental states.

While most of these points clearly have an intrinsic philosophical relevance, the standard of judgment for the success of the study of animal cognition can only be that of the empirical sciences. A survey of the state of the art reveals that much progress has been made in this direction, both by field research and by laboratory work exploiting traditional conditioning procedures.

The comparison of different species appears to be particularly impor-

tant. By considering also species not closely related to humans it is easier to overcome the danger of projecting the contents of our minds onto the experimental nonhuman animals. Furthermore, studying different species in their natural environment allows for the investigation of the adaptive value of cognition.

While it is generally accepted that cognition emerges from the activity of the nervous system, we do not know how complex this system must be to implement mental processes. It is certainly more intriguing to find evidence of cognitive processes in honeybees than in apes, and this emphasizes the utility of investigating even simple organisms.

Since the late seventies, the term cognitive science came to denote an interdisciplinary effort to understand cognition. Up to now, the disciplines officially pertaining to cognitive science are human cognitive psychology, philosophy of mind, neuroscience, artificial intelligence, linguistics, and cognitive ethology. We believe that it is time for animal cognition to be considered a component of cognitive science in its own right. To reach an integrated view of cognition, both developmental and evolutionary aspects are essential. Animal cognition contributes to the former, and is indeed crucial for the latter. Moreover, by studying cognitive processes in an ethological perspective, research on animals may shed light on the coupling between a cognitive system and its environment, thus introducing into cognitive science an ecological component that is still lacking.

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ANIMAL COGNITION AS PART OF COGNITIVE SCIENCE: A FRINGE ACTIVITY?

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This paper on "The mind of organisms" by Prato Previde, Colombetti, Poli and Spada provides an excellent account of the aims and assumptions underlying contemporary research in animal cognition. It puts forward a point of view that most researchers in the area today would probably share, but rarely make explicit. Among a behaviorist minority, who do not subscribe to such views and are unsympathetic to the whole idea of "animal cognition," there is still a strong suspicion that "cognitive" denotes both a loose anthropomorphism and a highly regrettable relapse into dualism. Consequently, what the present paper identifies as the two fundamental assumptions of the cognitive approach they endorse need to be emphasized again and again: first, that cognitive processes "are fully realized in the nervous system of the organism"; and, second, that they can be described at an abstract level that makes no claims about subjective experience nor the underlying neural events.

The paper ends with the call for animal cognition "to be considered a component of cognitive science in its own right." In making this claim the authors may well be pushing against an open door. I doubt that many cognitive scientists object in principle to the inclusion of natural, but nonhuman, cognition within the science. The question is whether it is likely to be, or should be, an important element. What grounds might one have for including animal cognition when, for example, planning a new course on cognitive science for a degree course? What does animal research have to export to its fellow members of the community of cognitive science?

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MENTAL HYGIENE

The paper suggests two related answers to this question. One may be termed the mental hygiene argument. The strong tradition of experimental analysis in animal psychology and the need to operationalize theoretical assumptions and implications, so that some measure of the behavior of a nonverbal organism can be used to test a theory, provide very strong encouragement of precision and conceptual clarity. This tradition and this need provide protection against the illusory belief that by invoking familiar concepts from folk psychology an explanatory account of some process is achieved. However, a similar argument has been employed by those impressed by computer analogies to emphasize the need to express psychological theories in terms of computer programs or, more recently, of neural network models (Rumelhart & McClelland, 1986). For many areas in cognitive science, notably those in which human language is involved, computers rather than nonhuman animals will continue to be seen as the preferred means for ensuring mental hygiene. We need to be clear about what distinctive contribution animal cognition can make in this respect.

INTENTIONAL ACTION

One very promising example is included in the paper. This is the research by Dickinson and his colleagues on the processes underlying instrumental conditioning. Lever-pressing by a rat in a Skinner Box has been the archetypal behaviorist preparation. Now it appears that an explanation of why this occurs may need to include both a belief (that this action is followed by a certain outcome) and a desire (currently wanting this outcome). However, this alone is unlikely to enrich theories of human behavior. A remarkable finding that may well have important implications for theories of human action is what Dickinson refers to as "incentive learning." As noted in the present paper, a change in motivational state from hunger to thirst, for example, may not be sufficient to produce a switch in behavior towards actions that in the past produced sucrose solution and away from those that produced dry food pellets. Before this occurs, the animal has to learn that sucrose solution is something to be desired when thirsty. Balleine and Dickinson (1991) have recently found that incentive learning is also necessary for an appropriate change in instrumental behavior to occur following the devaluing of a reinforcer resulting from the use of a conditioned flavor aversion procedure. Thus, a rat that has learned to press a lever for sucrose, which has then been followed by a lithium injection, will subsequently press the lever in extinction as frequently as a control animal injected with saline, unless it has an opportunity to experience the sucrose again and learn that it is now undesirable. If such incentive learning takes place, then a reduction in the instrumental action occurs.

This finding is particularly interesting in that comparable post-conditioning exposure to the reinforcer does not appear to be necessary for producing a change in classically conditioned behavior. Thus, in the test phase of Balleine and Dickinson's (1991) experiments rats that have not been given an opportunity for incentive learning nevertheless show decreased approach to the area of the dipper—a measure of classically conditioned behavior—as if at one level they "know" that sucrose has become aversive, even though their instrumental performance remains unaffected by the sucrose-lithium pairing.

At the very least, the incentive learning effect in instrumental performance shows that the same event, e.g., presentation of some sucrose solution in a dipper, is represented separately for instrumentally—as opposed to classically-based behavior. It is important to find out whether an analogous dissociation can be detected in human behavior. It may, for example, provide a new way of approaching the old problem of understanding the differences between rational action based on conscious belief and irrational attitudes, values, habitual responses and emotional reactions, of whose origins we are rarely conscious.

Whatever the outcome of future research on this topic these experiments provide a clear example of a domain of cognitive science in which research using animals is of great potential importance. It is difficult to imagine how such a distinction might have arisen from research using humans.

UNCLUTTERED FUNDAMENTALS

Which brings me to the second answer the paper makes to the question of why cognitive science in general should take note of animal cognition. This is the simplicity, or "uncluttered fundamentals," argument that has been used to justify inclusion of the study of nonhuman animals within psychology since the early days of this century (Boakes, 1984). Watson (1914) argued on these grounds for including comparative studies within psychology, as did, from a completely different perspective, both Koehler (1925) and Tolman (1932). For psychology as a whole there are plenty of concrete examples to support this argument, but for cognitive science compelling examples are thin on the ground. The flow of ideas had been very much in the direction of human to animal research. Despite a decade or more of substantial research on animal memory, for example, the impact on theories of human memory is hard to perceive.

Perhaps it is just a matter of time, so that in a few years hence the kind of research Dickinson's group are engaged upon, or current developments in the study of categorization (Pearce, 1988; Shanks, 1991) or of perceptual learning and latent inhibition (Hall & Honey, 1989) in animals will be seen to be as important for understanding human cognition as in an earlier era were Koehler's ideas on problem-solving and Tolman's on spatial learning.

In contrast, a very strong case for the current importance of animal cognition can be made on other grounds. It is one which Prato Previde and her colleagues omit to discuss. This is to provide a crucial link to neuroscience by means of animal models which allow exploration of the neural basis of cognitive processes. Current theories of memory processes may owe little to animal research, but our knowledge of what areas of the brain are important for various aspects of memory are largely based on animal studies (Aggleton, Hunt, & Rawlins, 1986). Understanding the attentional deficits shown in schizophrenia in terms of neurotransmitter balance is likely to be based both on the use of human cognitive tests and psychophysiological measures (Michie et al., 1990) and on the use of animal models such as the latent inhibition paradigm (Gray et al., 1991).

A "REAL" LEVEL OF EXPLANATION?

Rather than add further examples to what could be an impressively long list, I want to make it clear that the point of such examples is not to suggest that animal cognition is of value only to the extent that it contributes to research on the neural basis of cognitive processes. Prato Previde and her colleagues make some important points about different levels of explanation when, for example, contrasting pictorial with propositional representations and suggesting that both may be subsumed by some form of neural network theory. However, the further suggestion that explanations at a neural level are the only real ones and all others merely descriptions smacks of the kind of reductionism that makes particle physics the only true science. Just as economists and sociologists may develop entirely valid explanations for the phenomena that they study, without basing these on a psychological theory of the behavior of individual human beings, those studying animal cognition should strive to develop explanatory theories at an appropriate conceptual level, whether or not these can be related to events at a neuronal level.

The limitations of the behaviorist approach are well summarized at the beginning of the present paper. It is important to note, however, that the explanations offered by such theorists proved wrong in the face of behavioral evidence, not because of conceptual flaws or a failure to make contact with events at a neural level. One of the major achievements of behaviorist theorists was to show that one can develop explanations of behavior at a distinctive conceptual level, distinct both from the concepts of everyday folk psychology and from the level used in neuroscience. This is an important theme in Skinner's *The behavior of organisms*. It is not something to be thrown away in the process of substituting mind for behavior.

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THE REFLECTIVE MIND: AN ALTERNATIVE APPROACH TO ANIMAL COGNITION

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There is a curious double standard embedded in the paper on animal cognition by Prato Previde, Colombetti, Poli and Spada (this issue). They claim that "the main reason for attributing cognition to animals is that we, as humans, do experience a mental life" and furthermore "the prototype of cognition is, by definition, human thought." (p. 82) I agree that it is appropriate to inquire as to whether there might be cognitive states in animals that correspond to or at least resemble those of humans. Indeed, focusing on carefully targeted features of human experience could lead to some testable hypotheses about different features of the mental lives of animals, and as a consequence animal cognition might eventually become a matter of evidence rather than a matter of faith (Gallup, 1992). But a few pages latter, in the absence of any evidence, Prato Previde et al. erect some unnecessary barriers to this process by asserting that "reflecting on one's own beliefs and concepts" may be uniquely human and that the subjective quality of experience is "impossible to assess." (p. 87)

In their overview of animal cognition Prato Previde et al. focus on two paradigms, the "semantic" mind and the "computing" mind. There is, however, another approach, which I will call the "reflective" mind that focuses on the extent to which animals can conceive of themselves and represent mental states in themselves and others. If representation is the "central concept of cognitive theories" as Prato Previde et al. claim (p. 85), then it would seem reasonable to ask whether there are species capable of representing mental states in themselves and others. Prato Previde et al., however, dismiss this approach as an "anthropocentric mistake" and one that emphasizes abilities that may be exclusively human. (p. 87)

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But regardless of whether anthropomorphism is a mistake, it is a pervasive feature of human existence. People from all walks of life anthropomorphize. Indeed, as any radical behaviorist could testify it requires a lot of deliberate effort and training not to engage in anthropomorphism when describing the behavior of animals (Premack & Woodruff, 1978). So why do people anthropomorphize in the first place? From my perspective anthropomorphism is an inevitable byproduct of the "reflective" mind and represents an intriguing instance of mental state attribution in its own right (Gallup, 1985; Eddy, Gallup, & Povinelli, in press). Not only do people routinely make attributions and inferences about mental states in one another, in the case of anthropomorphism we simply generalize these mentalistic accounts of behavior to species other than our own.

Over a decade ago I theorized that the ability to recognize oneself in a mirror was based on an underlying capacity to conceive of oneself, and I reasoned that species that could correctly decipher mirrored information about themselves ought to be able to use their experiences and knowledge of their own mental states as a means of inferring comparable states of mind in other organisms (Gallup, 1982). This model implies that there may be animals which not only make attributions about mental states among one another, but just as we anthropomorphize by routinely generalizing such attributions to other species, they may do the same (Eddy, Gallup, & Povinelli, in press; Povinelli, in press, b). We should not be too quick to dismiss the presence of certain complex mental states in animals on the grounds that they may be uniquely human. Whether other organisms can conceive of themselves and use their experience as a means of modelling the experience of others is an empirical question and a variety of techniques are now available which can be used to assess mental state attribution in animals (Cheney & Seyfarth, 1990; Povinelli, Nelson, & Boysen, 1990; Premack & Woodruff, 1978).

Another important distinction among these approaches is that the "semantic" mind and the "computing" mind are largely descriptive/metaphorical accounts of animal cognition. The "reflective" mind differs from these not only in the cognitive processes that are targeted for analysis, e.g., self-conception, cognitive empathy, visual perspective taking, but unlike the others it provides a framework from which one can generate testable hypotheses about animal cognition (Gallup & Povinelli, in press). As Povinelli (in press, a) has recently pointed out, progress in the field of animal cognition is increasingly dependent upon replacing the "description-to-argument cycle" by a "prediction-to-data collection cycle."

In support of predictions derived from the "reflective" mind, there is growing evidence that points to some striking species differences between chimpanzees, that can recognize themselves in mirrors, and rhesus monkeys and other macaques, that fail tests of self-recognition in their ca-

pacity for mental state attribution (Cheney & Seyfarth, 1990; Povinelli, Nelson, & Boysen, 1992). For example, whereas chimpanzees can distinguish among different people as informants as a function of whether they have witnessed certain events (Povinelli, Nelson, & Boysen, 1990), monkeys seem incapable of taking into account knowledge states in humans (Povinelli, Parks, & Novak, 1991) and in other monkeys (Cheney & Seyfarth, 1990). It is also interesting to note the presence of some intriguing parallel changes in social cognition among human infants who begin to appear at or after the time they come to recognize themselves in mirrors (Brownell & Carriger, 1990; Lewis et al., 1989). As further evidence for its breadth and utility, the "reflective" mind has been used to provide an account of the evolution of human ethical systems (Povinelli & Godfrey, in press) and even the emergence of theistic thought (Maser & Gallup, 1990).

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ASK NOT WHAT'S INSIDE THE HEAD, BUT WHAT THE HEAD'S INSIDE OF

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Under the influence of behaviorism, much of comparative psychology became what could be characterized as "animal psychology." Evolution theory was not taken seriously enough, and "Cardboard Darwinism" passed for serious theoretical discussion. Evolution was believed to be the result of selective, environmental forces that act upon passive organisms. Under the influence of reductionism, the comparative psychology of the brain became what could be characterized as "the psychology of the hypothalamus." Again, evolution theory was understood simplistically, as a gradual, incremental process—a matter of degree.

The article by Prato Previde, Colombetti, Poli and Spada is part of a trend away from behaviorism and reductionism, toward a cognitive approach. The authors mention two related benefits of this trend: The introduction of a cognitive approach into comparative psychology would result in a fuller understanding of animals as active organisms with genuine reflective capacities; and, by the same token, the introduction of an evolutionary, or comparative, approach into the cognitive sciences would result in a more ecologically valid understanding of cognition—"which is still lacking"(Prato Previde et al., this issue). The authors assure us, though, that studying the reflective capacities of animals need not "imply a straightforward transfer to animals of models of human thought."

Unfortunately, I feel that there are still some serious concerns about the application of "cognitivism" to comparative psychology. My first concern is the impression that cognitivism is the only alternative to behaviorism. It isn't. Comparative psychologists interested in Integrative Levels Theory (Tobach & Greenberg, 1984), Activity Theory (Tolman,

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1987) and the Ecological Approach (Gibson, 1979; Gottlieb, 1985) have all provided similar approaches that avoid the problems of both behaviorism and mentalism. By not mentioning these other approaches, Prato Previde et al. give the impression, perhaps unintentionally, that cognitivism is the only alternative.

My second concern is more serious, and stems from problems with cognitivism itself. Cognitivism attempts to explain the activity of organisms on the basis of cognitive processes, i.e., solely on the basis of what is "in the head." But, by restricting the object of study to what is "in the head," the cognitive approach lacks ecological validity. Incidentally, I do not see any serious consideration of evolution theory in the cognitive approach either. How can an approach so seriously lacking in ecological validity be applied to comparative psychology? Without modification, without "ecologizing," the application of cognitivism to the study of animals may be inappropriate. Hopefully, through the process of application itself, cognitive psychology will become more ecological. But the authors give only a vague, almost token reference to this problem; they do not seem to realize the risk that an ecologically void methodology may actually distort our understanding of animal behavior.

My final concern, which follows from the second, is the one that concerns me the most. The cognitive method, I fear, does indeed distort! The whole method and aim of cognitivism, i.e., to seek out mental representations, functions as an *a priori* assumption that tends toward anthropomorphism: In order to apply the cognitive method to the study of animals, one must already assume that animals share basic cognitive processes with humans. In fact, if the whole aim of cognitive psychology is to explain behavior in terms of what is inside the head (i.e., hypothetical "cognitive states and processes"), then cognitive psychology cannot explain anything unless these states and processes really exist. Hence, by asserting that cognitivism can be used, one is asserting, *a priori,* that all animals, including insects, possess cognitive states and processes.

Of course, organisms are able to reflect their environment, and the psyche has evolved through various levels. But this in no way implies that the psyche is solely "in the head," nor that an explanation in terms of mental states is always the best one. My main concern, here, is that there is an opposition to Morgan's cannon inherent in the cognitivist methodology. Cognitive psychologists are more prone to construct some hypothetical cognitive process (e.g., cognitive maps) than to look for some ecological explanation (e.g., the available, biologically-relevant information). Prato Previde et al. warn that explanations in terms of mental states should not be *post hoc* re-constructions; my warning is that these same explanations should not be *a priori* pre-constructions!

One can see the effect of the *a priori* assumption in the way cognitivists tend to interpret results. They assume, before all else, that some mental

process can explain the given result. Hence, when a bee fails to recruit other bees to fly over water, we are supposed to assume that they possess in their tiny heads (in their tiny ganglions) some sort of tiny cognitive map of the surrounding area. When bees are able to differentiate between two flowers whose vertical orientation changes, but are unable to differentiate when the flowers' horizontal orientation changes, we are supposed to assume the existence of one mental transformation ("verticle transformation") and the lack of another ("horizontal transformation"). I assume that this same method can be applied to the catfish that was accustomed to going around a barrier on its way to a food source (Leontyev, 1981). When the barrier was removed, the catfish persisted to detour around the, now nonexistent, barrier. Are we to explain this behavior by assuming that the catfish "mentally represented" the barrier in its absence? If so, then we should also conclude that dogs, which go directly toward the food once the barrier is removed, are not privileged with this mental capacity! When one sets out to look for mental processes (and restricts one's object of study to mental processes) then one is setting oneself up to be fooled. Attempts to explain these phenomena in terms of "neural networks" does not help any, for this is even more restricted to what is solely "in the head."

My concerns are directed more toward the whole cognitive approach than toward Prato Previde, Colombetti, Poli, and Spada. It is a "wholesale" application of cognitivism, fallacies and all, that I fear the most. Instead, we should learn from the mistakes of cognitivism—not apply them.

The title of my commentary is taken from William Mace's (1977) essay on the ecological approach. Its advice is wise, for what exists in the head of organisms evolved as a reflection of the environment and of the organisms' activity within that environment. Indeed, it is impossible to study what is in the head, without studying what the head is inside of. But I will have to leave it to those theorists interested in the ecological approach, activity theory, and integrative levels theory to explain it themselves. It is their research that provides a truly alternative approach to behaviorism and mentalism.

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COGNITIVE SCIENCES AND THE MIND OF ANIMALS

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The paper by Prato Previde et al. discusses the problem of the cognitive approach to animal research. Are these studies condemned to remain within a strictly behaviorist framework? Can we escape from the S-R system without being accused of anthropomorphic interpretation? The reply to these questions requires that the criteria used to classify research as cognitive science be specified. It should also point out the difficulties likely to be encountered by anyone who raises the question of whether all animals are suitable for a cognitive approach.

Cognitive sciences include a system of information processing made up of data bases and processing procedures that may be activated by external stimuli, proprioceptive stimuli or by internal pathways. This system is capable of learning. The data bases can be enlarged and restructured, old procedures can be modified and new processes acquired.

The researcher's approach is thus to postulate the existence of mental structure and processing procedures, and then the model thus constructed must be sufficiently accurate to allow behavioral predictions. The reasoning behind the description of structures can be described as follows: if a mental structure (animal or human) has a specific characteristic, then in specific circumstances the individual should behave in a specific fashion. The reasoning used in studying processing procedures is similar: If the subject operates in a specific way, then under given circumstances the individual will behave in a particular way. Empirical data are used to test the validity of the prediction, bearing in mind that theses structures and procedures are biologically inscribed in the brain of the individual. As connectionist studies have shown, the neural organization serves as a model to describe cognitive structures.

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The problem raised by this approach to research on animals is summed up in the following four questions:

Do animals have mental structures?

How is nonsymbolic representation organized?

How can anthropomorphism be avoided when studying these structures?

How can we apply the cognitive approach to the themes that are central to studies of comparative psychology? The authors identify motivation, belief and desire as examples of these themes.

The authors answer the first question in terms of "mental state." Those who study cognition tend to speak of representations; these form one category of mental states. The question then is to decide whether animals have representations. The most basic representations are those that allow recognition of objects. To recognize prey (or more generally, anything that can be eaten), an animal must have stored in memory a representation of this prey. The representation must have the qualities of the prey without in anyway being dependent on nonsignificant details. This would indicate that all animals that show by a specific behavior that they recognize a stimulus (animate or inanimate) have representations, i.e., data bases containing the main characteristics of stimuli that have significance for the animal. The problem for researchers is to determine if all animals have representations, and thus to define the criteria by which it can be said that a given species does not have representations and only responds to stimulations. This focus on the notion of representation does not mean that studies on nonrepresentational capacities should be neglected as these play a major role in certain species.

Assuming this to be so, then, as animals have no verbal language, do they use representations that are not symbolic? Cognitive psychologists have already encountered this type of representation as indicated in the studies of Rumelhart and McClelland (1986) on perception. There remains the methodological difficulty of studying representations in an individual that cannot express itself via a verbal language. Cognitive psychologists are aware of this problem. The research methodology used in situations in which the individuals has no language is now well established. We should remember that the same problem is encountered in human studies, especially in studies of infants, where researchers have developed experimental paradigms, such as habituation (Gottlieb & Kranesgor, 1985), that allow, for example, studies of perception in newborns. The lack of verbal responses does not constitute a major difficulty, even if it increases the risk of anthropomorphic interpretation of results. This danger remains, however, limited if the researcher is careful to predict the empirical consequences of particular mental model, and to compare the functions of animal mental models with those of human mental models. In this sense, it can be said that animal psychology is essentially comparative.

The last question concerns the cognitive approach to motivation, belief and desire. These areas of research have only been recently treated in cognitive psychology. It is thus an area in which cognitive psychologists have much to learn from animal studies. The current approach (Martins, 1985; Nelson, 1988) involves the action of motivation, belief and desire on representations. This problem can readily be transposed to comparative psychology studies.

In conclusion, as stated by Prato Previde et al., there is no major difficulty in studying animal cognition. But, researchers should nevertheless define the level of the evolutionary scale at which it can be assumed that animals have representations. This question indicates that we should define the cortical structures necessary for the development and the storage of representation.

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TOWARDS A COMPARATIVE AND EVOLUTIONARY APPROACH TO COGNITION: REPLY TO COMMENTARIES

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The four commentaries are interesting in two respects: first, in that they raise a number of further issues about animal cognition; and second, because they show how many different positions can be entertained on this matter. While Christopher Robinson clearly rejects the cognitive approach as an alternative to behaviorism, Robert Boakes has no basic objection to animal cognition, but focuses on its actual contribution and reminds us that the positive legacy of behaviorism should not be thrown away with the bath water. Within a comparative approach to cognition, Gordon Gallup suggests that the best framework for generating testable hypotheses is the study of complex mental processes like self-recognition and reflective thought. On the other hand, Jean Pierre Rossi raises the problem of identifying the lowest evolutionary level at which the existence of representations can be assumed. In the following, we shall try to briefly discuss the main points that have been raised.

Christopher Robinson takes up William Mace's suggestion to "Ask not

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what's inside the head, but what the head's inside of." Let us emphasize that we see no reason why one should ask just one of these two questions, and not both. The fact that our paper was about the first one does not imply that we take the other to be less relevant: we believe that no single approach is able today to provide a complete explanation of behavior. Anyhow, at least human cognition is a matter of fact requiring an evolutionary explanation. Certainly, evolution theory is not seriously considered in present day cognitive science, but we believe that an innovative contribution in this direction could be brought about by a comparative perspective.

The cognitive approach does not restrict the object of study to what is "in the head," but rather it extends the study of behavior to what is "in the head." Certain branches of cognitive science, and notably artificial intelligence, have indeed restricted their attention to internal processes; but the literature of animal cognition and cognitive ethology already shows a more integrated view that takes ecological aspects into account. After all, adaptation and evolution have always been central issues in comparative psychology; animal cognition does not break with this tradition.

According to Robinson, cognitivism contains "an *a priori* assumption that tends toward anthropomorphism." Cognitive scientists rely on *a priori* assumptions as all other scientists; there is nothing vicious in this, provided that such assumptions are used to generate hypotheses that can be empirically tested. As regards anthropomorphism, we do not view it as a tendency, but rather a source of inspiration for building theories; and we should note that the scientific procedure is to verify the predictions, and not the origin, of a theory. Moreover, to look for similarities and differences in mental processes of different species is well within the tradition of comparative psychology, and fits an evolutionary perspective.

The controversy on the location of the psyche is too vast to be dealt with here in sufficient detail. By their very nature, mental states have a content and thus presuppose an external world; nevertheless, they are states of a precisely delimited physical system, namely, the brain, that happens to be located in the head. And as regards the environment, by itself it does not contain information, but mere physical processes that become informative when an organism can interact with them in an appropriate way. The reason why polarized light carries relevant information to bees and not to us is not to be found in the environment, but somewhere inside the bees' tiny heads.

Robert Boakes gives a clear summary of the reasons in favor of a comparative approach to cognition, and emphasizes a further point, namely the crucial link that animal models provide to neuroscience.

As regards the "mental hygiene" deriving from the experimental tradition of animal psychology, Boakes remarks that in several areas of cognitive science computational models are considered as a safer tool. In

our opinion, however, the experimental tradition and computer simulation should be regarded as complementary rather than competing. Correctly used, computers can improve the "hygienic conditions" of research, but by themselves they are unable to assess the correspondence between theoretical models and real phenomena.

We agree with Boakes on the importance of the behavioristic legacy, i.e., seeking explanations of behavior at a conceptual level distinct both from the neural level and from everyday folk psychology. In no way can mind substitute for behavior: it is a possible source of explanations for behavior, as well as being itself something to be explained.

Clearly, mental explanations require that certain constructs, used to explain other processes, are taken as elementary and unexplained. For example, the behavior of a given organism could be explained assuming that the animal stores certain cues in memory; but then, the capacity to do so is taken as a primitive. In turn, primitive processes might find an explanation at a lower level: it is in this sense, we believe, that the neural level can explain capacities that at a cognitive level can only be described. A similar relationship between two levels of explanation is common in science, and is well exemplified, for instance, by the relationship between thermodynamics and statistical mechanics.

From our standpoint, however, the main contribution of animal cognition to cognitive science is the phylogenetic and comparative perspective that it brings in. The point is not only to shed light on human cognition through the study of animals, but rather that if cognition is a natural phenomenon with an adaptive value, it plausibly extends beyond the human species.

Gordon Gallup suggests that, besides the two approaches to cognition we have discussed, there is a third approach, which he refers to as "reflective mind." It seems to us that this is not a different approach, but rather a specific and extremely interesting area of investigation, that can only be tackled from a cognitive perspective. Reflective representations are a specific type of representations, and, therefore, have to be interpreted either as mental states with content or as IPP structures.

Gallup quite correctly remarks that the reflective mind hypothesis provides a framework from which interesting testable hypotheses can be generated. However, such hypotheses presumably apply only to primates, and not even to all of them; as interesting as this area may be, it does not exhaust the field of animal cognition. The paradigms discussed in our paper are intended to cover a broader range of phenomena; moreover, the research by Dickinson and his colleagues, as well as other studies reported in the literature, suggest that both the semantic and the IPP approaches are suitable for experimental investigations, and thus are no mere metaphors.

The urge to broaden the scope of animal cognition justifies our claim that ". . . self-awareness and thinking about the process of thinking itself

are by no means necessary components of cognition." Of course, this does not mean that they should not be regarded as possible components, and well worth studying. We agree that they should not be too quickly dismissed—the history of psychology shows, however, that sometimes they have been too quickly accepted.

The focus on the reflective mind is an example of a positive product of anthropomorphism, that is the use of human experience as a source of testable hypotheses on other species. But this does not rule out the impossibility to grasp the phenomenology of the experience of other organisms, which is a consequence of the subjective nature of the mind.

Jean Pierre Rossi attributes to us the opinion that there are no major difficulties in studying animal cognition. Perhaps we are not that optimistic. We believe that there are in principle no methodological grounds to reject models of animal behavior based on cognitive constructs, and that establishing which animals, if any, have mental processes is a theoretical question of fundamental importance, that deserves an intensive research effort. However, this path is not without difficulties. It is true that similar problems are faced by those who study newborns or infants in a prelinguistic stage; but here we are still dealing with the human species, of whose mental life we are fairly confident.

It is not possible to define *a priori* at what evolutionary level the cognitive approach can be profitably applied. Representations cannot be directly observed; therefore, the initial evidence that representations do exist can only derive from the success of models that postulate them to explain behavior. As remarked by Rossi, it is the predictive power of models that has to be assessed; only after sufficient evidence of this kind is available, will it be possible to correlate the existence of representations with the complexity of the nervous systems, the existence of cortical structures, etc. What we know today is that our neural structures are sufficient to realize cognition, but we do not know what kinds of neural structures are necessary for representations to exist. Only a comparative and interdisciplinary approach can lead to positive results in this direction.

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BOOK REVIEW

Experimental studies of elementary reasoning: Evolutionary, physiological and genetic aspects of behavior, L. V. Krushinsky. Translated from the edition, 1986, posthumously edited by A. F. Semokhina. English translation edited by Ethel Tobach and Inge I. Poletaeva. Published for the National Library of Medicine, Bethesda, Maryland by Amerind Publishing Co. Pvt. Ltd. New Delhi, India, 1990, XXII + 311 pp.

Closing a chapter on animal cognition, Terrace (1984) referred to a "baffling but fundamental question." "Now that there are strong grounds to question Descartes' contention that animals lack the ability to think, it is appropriate to ask, how does an animal think? . . . Learning the answer to that question will provide an important biological benchmark against which to assess the evolution of human thought." (p. 22)

Experimental studies of elementary reasoning: Evolutionary, physiological and genetic aspects of behavior, by the Russian ethologist, Leonid V. Krushinsky (1911–1984), describes the resolute attempt of himself, his colleagues and students at the Moscow State University, to face the "baffling but fundamental question" of animal thinking in its elementary forms. It belongs to an old tradition in the comparative study of behavior, which goes back to Romanes, Morgan and others, but also to Russian comparative psychologists, such as Wagner, who were all concerned with the stages that paved the evolutionary way to human reason. Written clearly, full of challenging ideas, the book represents the culmination of a long research career dedicated to the study of elementary reasoning, i.e., problem solving in animals. Krushinsky's range of interests was impressive. He approached the question of reasoning from several perspectives: behavioral, neurophysiological, anatomical, genetic. He described behavioral episodes in natural environments, discussed the relationship between reasoning activity and social behavior and speculated about the evolution of brain and behavior.

All this does not result in a potpourri of findings and discussions. There is a fundamental unity in the book, each chapter bringing a new perspective that is integrated with the rest. Because of the scope of the work, all topics are not treated with the same depth, and sometimes one

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would welcome more information and more analysis. While following Krushinsky in his formidable trip, I caught myself several times inventing experiments that would reveal aspects not investigated or trying to tackle conceptual issues, such as redefining reasoning in representational terms. But this is exactly what makes the book stimulating.

ON THE NATURE OF ELEMENTARY REASONING

Krushinsky's proposal starts with an essential assumption: elementary reasoning does not derive from conditioning. Although in concrete acts elementary reasoning may be closely integrated in conditioning or instinct, it cannot be reduced to either. It has to be considered as a behavioral module, with its own causal bases and its own ontogenetic course. Contrasted with learning that requires repetition of trials and is generally marked by a gradual improvement in performance, elementary reasoning involves immediate adjustment to an essentially new situation.

Reasoning goes beyond perception. Its essential property is the capacity to apprehend the empirical laws that connect events and objects in the outer world. Of course, animals are not physicists, and lack the Pavlovian second signal system that would allow them to understand the surrounding world and express theoretical laws. They use empirical regularities, however, in order to anticipate events. Among the simplest regularities are the following: (1) an object continues to exist, even when it disappears from the preceptual field; (2) opaque objects are impermeable to locomotion; (3) an object can be enclosed in another tridimensional, hollow one; (4) such an object moves when the object into which it is placed moves.

EXTRAPOLATION AND DIMENSIONALITY TASKS

Extrapolation is the ability of an individual animal to predict the future location of some part of the environment, on the basis of contemporaneously perceived change. Several tests were devised to assess extrapolation ability. The screen technique involved offering food to a deprived animal, in one of two bowls placed in a gap between two vertical, opaque screens. After some ingestion had occurred, both bowls were displaced, one of them to one side so that the animal could see the trajectory of its movement, the other to the opposite side; the animal could hear the movement, but not watch as it moved behind the screens. To correctly solve the problem, the animal had to go around the screen behind which the bowl with food disappeared. Displacement of the empty bowl served as a control for sound.

According to Krushinsky, the initial perceived movement of the food bowl provides information on the parameters of movement that is transferred (extrapolation) and that makes selection of the correct response

possible. Control for learning is obtained only by taking into account the initial performance in the problem solving situation. Later performance is contaminated by reinforcement effects and does not represent true reasoning.

Krushinsky and his co-workers compared the performance of a number of animals on extrapolation tasks: fishes, frogs, turtles, tortoises, lizards, birds, rats, rabbits, foxes, wolves, etc. It is refreshing to see such a range of species invading the traditional territory of white rats and pigeons. In some animals (fishes, frogs, rabbits, fowl), reasoning ability was quite poor: animals would search a long time near the place where the food disappeared, sometimes gaining access to reward through trial and error; lizards, tortoises and turtles (who would predict turtles could solve problems?) showed significant proportions of correct choices; crows and magpies confirmed the good reputation of the Corvidae; cats and dogs reached food with "quick, purposeful movements."

In dimensionality tasks, which were more abstract and demanding than extrapolation ones, animals had to infer the principle of containing or being contained. A typical procedure with dogs was to let the animal feed from a container, placed near two objects, one of them tridimensional, such as a pyramid and the other one two-dimensional (flat), such as a triangle. The bait was then hidden in the tridimensional object, out of the view of the dog. Both objects were subsequently displayed to the animal and put in different locations. The immediate choice of the tridimensional object indicated knowledge of the basic regularity: a tridimensional object cannot be contained in a flat one.

Dogs and wolves, although very good at extrapolating, were not able to solve the dimensionality problem. In order to test dolphins, an aquatic version of the setup was used with two female dolphins, Vasilisa and Malyshka. Instead of food, a ball with which the animals liked to play was used as a reward. At first dolphins showed that they had chosen a stimulus by a very interesting and indirect way: splashing water at it. Afterwards, experimenters decided that a more conventional operandum, a pedal attached to the object, should be used. The dolphins significantly selected the correct (hollow) object, from the very beginning. *Maccaca, Cebus* and *Cercopithecus* species of monkeys and bears (bringing a Russian flavor into the experiments) also gave excellent performances. Control experiments showed that such results were not due to a baseline preference for tridimensional objects.

What about human beings? A longitudinal study with children from 1 year 6 months to 4 years of age, by Moldkina, Kadrybaeva and Obukhova showed an increase, with age, in the proportion of correct responses to a modified screen experiment but, curiously enough, only under a multi-trial procedure. When asked about the toy, younger children would give a nonexistence answer such as: "Not here; gone away; I don't know where it is now." (p. 86) They would insist on staying near the place

where the toy was last seen. At four, most children displayed active search and correct responses.

Behavioral disturbances and physiological arousal occurred, during problem solving, a very interesting observation. Some crows, after several correct extrapolation responses, acquired an intense fear of the apparatus, a kind of phobia. They would avoid eating altogether near the screens. Dolphins, when tested more than two times a day, showed a decline in performance, stereotypically choosing the same side of the screen. Krushinsky's opinion is that reasoning tasks can cause strain, especially when difficult. The paradoxical aspect is that emotional disturbances mostly occurred after successful performance. How can our learned helplessness theories account for that?

THE "MATERIAL BASIS" OF REASONING

In several chapters of the book, behavior is related to neurones and neurones to genetic factors, in a way that verges on reductionism. Evolution of reasoning is based on the progressive development in size and complexity of association areas of the brain. Krushinsky claims that the "excess of potential capacity of the brain" is a condition both for adaptation to environmental diversity and for the emergence of higher level behavioral capacities. Evolution would thus have preadapted organisms by selecting "useless" amounts and complexities of neural tissue. This issue is controversial. It took me back to an argument by Kaplan (1987) in support of "extra-units" in the nervous system. According to Kaplan, "extra-units make possible patterns of activity that can function in an 'as if' mode without restricting the organism to the immediately present environment and without necessarily leading to motor output." (p. 672) In other words, extra-units are a prerequisite for representation and for thinking.

Some interesting neurophysiological research is reported, which gives supplementary support to the learning/reasoning modularity. The dorsal cortex, the reptile brain structure most probably involved in cognitive processing, was eliminated in groups of pond tortoises which solved the extrapolation task successfully, but had different levels of training (Ochinskaya). Ablation had a very significant effect in animals with little training, bringing performance to chance level. In groups with extensive training, on the contrary, the number of correct responses remained high after the operation. Such results can be accounted for by supposing that extrapolatory and learning performances depend on different neural circuits. Dorsal cortex is essential for the initial responses to a new problem situation, but its integrity does not matter, once control is assumed by conditioning.

Similar results were obtained with crows: destruction of cortical structures involving the hyperstriatum reduced extrapolation ability in ani-

mals without previous training, but not in those previously trained. Dogs and cats that had prefrontal regions of the cortex removed required much longer time than sham operated animals to show correct extrapolation performance. Such results point to homologies in brain structure and function, and deal with the important question of the neural circuit as endowed with a "generation of predictions function." (Gray, 1984)

GENERAL ASPECTS

Lockhard's (1971) criticism of comparative psychology included the argument that white rats were poor, degenerated copies of wild rats. A number of experiments, done afterwards, showed that white rats were not such poor copies after all and that they retained both the essential species-typical responses, and the ability to master complex learning situations. Results of Krushinsky's genetic experiments bring back part of the old interpretation. While laboratory rats of several strains were unable to solve an extrapolation problem, brown rats reached the respectable score of 82% correct solutions. Hybrids of brown and laboratory strains had an intermediate performance.

On the basis of such results, and of similar ones obtained with foxes of different strains, Krushinsky draws a Lorenzian conclusion about the bad effects of domestication. By increasing the probability of phenotypically extreme types and by promoting genetic drift effects, domestication would induce degeneration in genetic systems. "In wild animals, the level of their elementary reasoning is developed so that it is most adapted to the conditions of their existence. Naturally, the process of disintegration of the integrated genotype in the course of domestication results in the reverse, in the lowering level of reasoning." (p. 147) Many of those who study cognitive processes in albino rats may disagree with such a conclusion.

OBSERVATIONS IN NATURAL CONTEXTS

I enjoyed the part of the book dedicated to field observations and to the spontaneous, natural occurrence of reasoning behavior (cognition is not only something psychologists reveal under the very special conditions of the laboratory) regretting only the small amount of systematic original findings.

The case I found most interesting is that of woodpeckers which insert pinecones in existing holes in trees, in order to extract the seeds. The holes can be used for a long time, day after day, so their use as clamps or vises is not an accidental one, but a matter of habit. Pinecones are transported, fitted into the crevice and seeds dug out. All this is indicative of "tool using" and reminds one of the nutcracking behavior of chimpanzees at Tai, Ivory Coast, or at Bossu, Guinea, also characterized by

the choice and transport of both "tools" and nuts (Boesch & Boesch, 1984; Sakura & Matsuzawa, 1991).

Do birds use several trees simultaneously, shifting from one to another according to spatial distribution of resources or size of the cones? Can they perceive the relationship between size of the cone and size of the hole? Krushinsky used a wooden wedge several times to close a slit that was used by a great spotted woodpecker. The animal would take out the wooden wedge or would make and widen new holes by pecking at it. More than such casual observations are of course needed to support Krushinsky's conclusion that "the great spotted woodpecker can 'construct' a vise for pecking cones and arrange it in such a way that the clamp's shape corresponds to the shape of cones to be inserted." (172–173)

SOCIAL BEHAVIOR

The richness and variability of social behavior depends on the development of elementary reasoning ability. According to Krushinsky, both aspects showed a parallel, mutually invigorating increase, during evolution, while instinctive signal systems receded and were substituted by individual knowledge of others among members of a community. This relationship between cognition and social life is one that arouses enormous interest in the new generation of ethologists and comparative psychologists.

In crows, the following instances suggest cognitive involvement: exploratory behavior, manipulation of sticks and other objects, play episodes in which one animal chases another one, stealing an attractive object from its beak; vocalizing episodes such as the "roll-call" during which crows alternate singing episodes, learning perhaps to identify individual calls; the establishment of a dominance hierarchy, etc.

In the description of animal communities, Krushinsky puts a greater emphasis on cooperative aspects than on agonistic ones. Typical of this emphasis are statements such as "community (of African hunting dogs) is organized on the basis of extremely 'friendly' relationships" (p. 202), "aggression does not play a decisive role in maintaining the structure of communities of jays" (p. 196), "rather than aggression, personal 'sympathy' between individuals of the same as well as of the opposite sex represented a more essential factor in the life of a community of wolves." (p. 201) Aggression's basic function is a dispersal promoting one, it puts distance between groups and gives origin to exiles that transmit genes from one community to the other. Elementary reasoning, on the contrary, is one of the bases for mutual assistance and cohesiveness, and, thus, a very powerful instrument of evolutionary change.

The importance given to cohesive social tendencies derives from Krushinsky's belief that evolution proceeds principally through group

selection. Individual or collective behavior that result in benefit for the community are inevitably selected, through group selection: selfish behaviors can only spread under individual selection. Both principles are perfectly true, but also, only true in principle. Modern views give high priority to individual selection, and offer a very different, somewhat harsher and more Hobbesian picture of social organization.

COMPARISON AND THE ESTABLISHMENT OF SCALES

Using the scores of elementary reasoning tests as a criterion, Krushinsky ordered the species he studied in scales of progressive ability, taking separately each great taxonomic group (mammals, birds, reptiles, amphibians and fishes). Among mammals, for instance, we have, at progressively higher levels of ability: (1) rats and rabbits; (2) cats; (3) silver-black and arctic foxes; (4) red foxes, wolves, dogs, Corsac foxes and raccoon-dogs; and (5) monkeys, dolphins and bears.

Phylogenetic evolution of brain and behavior, according to Krushinsky, did not proceed in the same way in different taxonomic group. Inside each of them, however, animals can be ordered along a scale, according to the level of reasoning. Implicit is the anagenetic (Campbell & Hodos, 1991) idea that evolution is directional and progressive: "Progressive evolution involved the growing capacity of animals to apprehend a greater number of natural empiric laws."

The possibility of establishing hierarchical series of species, on the basis of cognitive ability, is not a consensual matter. Some extreme positions, such as MacPhail's (1987), according to which there are no fundamental qualitative differences among animals in intelligence (at least, among nonhuman vertebrates), are not convincing. But once one accepts the fact that frogs differ from dogs, how is one expected to analyze and interpret the differences?

I personally am not very comfortable with classification of species along scales, when such classification implies strict phylogenetic evolution. I prefer an ecological perspective, which consists in taking learning and cognitive abilities as adaptations to specific habitats. General processes are of course real, and indicate the fact that very different species had to cope with similar general constraints of the environment. The comparative method, however, can lead us to explore and respect diversity (including diversity in learning and cognition) and to relate it to the tasks and circumstances with which animals normally have to cope.

REASONING ABOUT THE ELEMENTARY REASONING CONCEPT

To what extent is the elementary reasoning concept reasonable? I think Krushinsky has pinpointed a real and very relevant aspect of behavior that deserves experimental and theoretical consideration in its own right. One key element of his position is the idea that some animals at least

are able to apprehend empirical laws, that is, causal regularities of the environment, and use such knowledge in response to selection. He is not explicit about the mechanisms that could mediate reasoning, and does not indulge in "representational model building," but his ideas are convergent with several recent cognitive proposals, such as Gallistel's (1989) views about functioning isomorphism between environment and brain processes.

I am concerned, however, with some limitations of the elementary reasoning concept and of its operational definition: (1) extrapolation and dimensionality tasks can, at best, assess one or a few aspects of cognitive abilities of animals: they cannot be taken as a primary test of reasoning. As Sherry argues (1987), intelligence may not be a single capacity, but a collection of capacities.

(2) Scores in any single task are not necessarily representative of the animal or species level of cognitive ability. Motivational, species-specific "misbehavior," and, most importantly, the structure of the experimental situation can influence performance, favoring or impeding correct responses. Pigeons, which are quite unable to solve food-behind-the-screen problems, may be quite clever at solving other complex tasks, depending on the current contingencies. Designing tasks that are ecologically relevant, meaningful from the point of view of the natural environment and of the animals' normal way of life could reveal unexpected performances: animals will function most readily in environments that resemble the ones to which they are adapted.

(3) A modular conception of elementary reasoning should not hinder an analysis of the influence of past experience and of learning on intelligent acts. New solutions are frequently the result of a reordering of old, learned behaviors, and some problem solving strategies can be enhanced through training. Exposing chimpanzees to language training, for instance, makes them more proficient in special cognitive tasks such as same/different judgments, solving analogies, etc. (Premack, 1983).

OMISSIONS

A final observation: I sought and did not find mentioned in the book the names of two psychologists, one who published relevant studies from the point of view of extrapolation. The first one is Piaget (1937) who investigated the way young children learn about several essential properties of objects, exteriority, substantiality, individual identity and permanence. Permanence is exactly what Krushinsky means by "the law of nondisappearance of objects." Piaget showed that the infant's search for a hidden object followed orderly stages, going from total indifference as the object ceases to exist when it leaves the perceptual field, up to the stage when the child is able to reconstruct, through ideation, the invisible course of the object.

The second omission is Etienne's (1973) study of object permanence

in chicks. Etienne, who knew about Krushinsky's works and was probably influenced by him, used a situation with two parallel, vertical screens, with a glass tube in between, in which chicks could see and follow a mealworm until it disappeared. In this setup, which is very similar to the tunnel or screen setups invented by Krushinsky, Etienne found that during the first trials the animals stayed near the place where the mealworm was seen for the last time, most of them giving distress calls. Later on they started going around the screens and learned how to find the bait. Performance was stimulus-bound: it dropped to chance when relevant aspects of the situation were changed. Such findings confirm Krushinsky's results and give support to his distinction between learning and elementary reasoning.

The fact that Krushinsky did not mention either Piaget or Etienne, (probably not knowing about their publications), and the fact that he himself is almost never quoted by American or European behavioral scientists, in contrast with, for instance, Vinogradova, is a fact that makes one reflect about the crucial importance of communication of results and of exchange of ideas in research, and makes one welcome the timely translation of the current book. Behavioral (and neurophysiological) work under Krushinsky's inspiration would surely not be dissonant in the current field of comparative cognition; it would fit, for instance, in the context of Piagetian studies of animal behavior (Dore & Dumas, 1987; Dumas & Dore, 1991).

Krushinsky's book, with all its scholarship, is about a single basic insight: the existence of elementary cognitive processes and the relevance of their study for an integral science of behavior. He wrote that a "great deal of experimental data exist, and important theoretical generalizations are being formulated concerning mechanisms of learning and instinct. But these two important components of behavior are not sufficient for formulation of a general theory of behavior. Such a theory requires a third component to be added—elementary reasoning." (xxii) His book represents a creative exploration of that third domain, made in the spirit and enthusiasm of pioneers. It is a thought-provoking book, written at the threshold of a new era in the comparative study of cognition.

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